The Two Ernests—I

Some personal recollections of Ernest Rutherford and Ernest Lawrence in the period 1927-1939. Rutherford, who dominated the Cavendish Laboratory, gave his physicists a minimum of equipment but a maximum of personal interest in their research. Lawrence developed the Radiation Laboratory into a prototype facility for research with large, expensive equipment. Both inspired others to produce and interpret nuclear reactions.

by Mark L. Oliphant

ON 11 JANUARY 1939 after a visit to Berkeley, I wrote a letter to Ernest Lawrence that contained the following paragraph:

"I find it very difficult to thank you for the magnificent and instructive time which I had in Berkeley. It was truly fine of you to be so liberal of time and of thought on my behalf. I know of no laboratory in the world at the present time which has so fine a spirit or so grand a tradition of hard work. While there I seemed to feel again the spirit of the old Cavendish, and to find in you those qualities of a combined camaraderie and leadership which endeared Rutherford to all who worked with him. The essence of the Cavendish is now in Berkeley. I am sincere in this, and for these reasons I shall return again some day, and I hope very soon."

Now, in 1965, after many subsequent visits to the Radiation Laboratory, which Lawrence created and which is now named after him, I remain intrigued by both the many similarities, and the differences, between Rutherford and Lawrence.

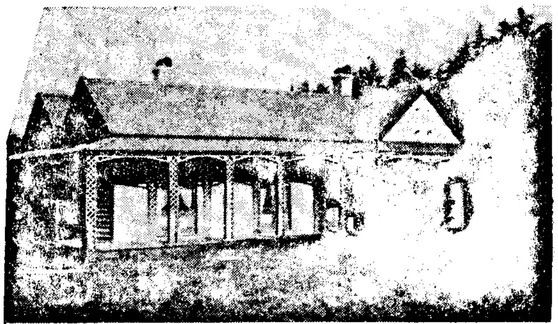
John Cockcroft and Ernest Walton first observed nuclear transformations produced by artificially accelerated particles, and James Chadwick discovered the neutron, in the Cavendish Laboratory, Cambridge, in 1932. Lawrence conceived the cyclotron principle in 1929, in the University of California, Berkeley. By 1932, with his colleagues Niels Edlefsen and M. Stanley Livingston, he had made the cyclotron a successful instrument with which he was able to confirm the results of Cockcroft and Walton, and carry them to much higher bombarding energies. The period between these great discoveries and that of the fission process by Otto Hahn and Fritz Strassmann in 1938, was of the greatest importance in the development of modern physics. In this article, I endeavor to set down some recollections of that period and of two individuals who gave it such momentum that it changed the whole course of physics and led, inexorably, to the development of nuclear weapons and nuclear energy. No pretense is made that this account is complete, or that the facts presented are in accordance with the recollections of others who lived through those stirring days. The study of the effects produced in the atomic nucleus by bombarding it with nuclear projectiles had transformed knowledge of matter and its properties. The parts played by Rutherford and Lawrence, directly and indirectly, will remain outstanding contributions to that work.

Ernest Rutherford and Ernest Lawrence, in two succeeding generations, built around them great schools of investigation that laid the foundations of physics as it is practiced today. These two men, so much alike, and yet so strangely different, were parts of totally different worlds. Together, their lives spanned the period of the greatest revolution in knowledge of the physical universe since Newton's time. Each was a pioneer, and each was the descendant of pioneering parents who chose to build a new life in a land far removed from the home of their ancestors. It is revealing to review the early life of each.

Rutherford, early years

Rutherford's grandfather, George Rutherford, migrated from Scotland to New Zealand in 1842. His son James, then three years of age, grew up in

Sir Mark Oliphant, K. B. E., F. R. S., worked with Ernest Rutherford in the Cavendish until 1937, when he went to the Univ. of Birmingham. In 1950 he became professor of particle physics and director of the Research School of Physical Sciences at the Australian National University.



HOUSE, in South Island, New Zealand where Rutherford lived as a child.

the colony and followed his father's trade as a wheelwright. James met and married a widow, Caroline Thompson, who had left England for New Zealand with her parents, in 1855. They settled near Nelson, in the South Island, where James Rutherford had a small farm and worked as a contractor building the railways. Ernest Rutherford was born on 30 Aug. 1871, the second son in a large family of twelve children. When Ernest was eleven years of age, the family moved a short distance to Havelock, where his father established a mill to treat the native flax of the area, and a small sawmill. At the primary school there, Ernest was influenced by his teacher, J. H. Reynolds, who taught so well that Ernest won a scholarship to Nelson College, with almost full marks in the examination. He entered the College at 15 years of age, and was much helped by one of the masters, W. S. Littlejohn, a classicist who taught also mathematics and science. Ernest had a broad education, excelling in mathematics, but winning distinctions in Latin, French, English literature, history, physics and chemistry, and becoming head of the school. He was a scholar of distinction, but played games reasonably well and entered fully into the life of the school. A. S. Eve, in his biography of Rutherford, quotes a fellow student as saying, "Rutherford was a boyish, frank, simple and very likable youth, with no precocious genius, but when once he saw his goal, he went straight to the central point." He took photographs with a home-made camera, dismembered clocks, made model water wheels such as his father used to obtain power for his mills. Under the influence of his mother and his fine teachers, Rutherford developed a wide taste for literature and read avidly all his life. He became especially interested in biographies.

In 1889 he won a scholarship to Canterbury College, Christchurch, a component college of the University of New Zealand. There, as one of 150 pupils in the small institution, he enjoyed five very full years, obtaining successively his B.A. and M.A. degrees, the first in Latin, English, French, mathematics, mechanics and physical science, and the second, at the end of his fourth year, with a double first in mathematics and physical science. During his fifth year, Rutherford concentrated on his science, carrying out many experiments on the electromagnetic waves discovered by Heinrich Hertz, and investigating the effects of the damped oscillations of the Hertzian oscillator upon the magnetization of steel needles and iron wires. He showed that the magnetization was confined to a thin, outer layer of the metal, by dissolving away the surface in acid.

Rutherford was able to use these magnetic effects to detect the wireless waves from his oscillator, and demonstrated that these waves travelled for considerable distances, passing through walls on the way. He reproduced Nikola Tesla's experiments on the high voltages that could be produced with a resonant transformer, and developed techniques for measuring intervals of time as small as 10 microsec. He spoke to meetings of the Science Society on his work and on the evolution of the chemical elements, and he published two papers in the *Transactions* of the New Zealand Institute. He found it necessary to supplement his scholarship by coaching students, and went to live with a widow, Mrs. de Renzy Newton, whose daughter Mary he later married.

In 1895 Rutherford applied for an 1851 Scholarship, which was awarded to a New Zealand student in alternate years. The examiners of the 1851 Royal Commission, in London, awarded this to a chemist, J. C. Maclaurin, but were impressed enough by Rutherford to urge the award of a second scholarship, which was not given. However, Maclaurin gave up the scholarship to accept an appointment in the civil service; so Rutherford was offered the award. He elected to go to the Cavendish Laboratory, in Cambridge, to work under J. J. Thomson, and had to borrow the money to pay for his passage to England. He and John S. Townsend, of gas-discharge fame, arrived at the Cavendish Laboratory almost simultaneously, to become the first of the new category of research student recently established in the University of Cambridge. There he joined Trinity College and began fresh experiments on the detection of electromagnetic waves by use of the effects of high-frequency currents upon the magnetization of iron wires. He soon established himself as a research worker of great promise, of whom Andrew Balfour wrote, "We've got a rabbit here from the antipodes and he's burrowing mighty deep." Rutherford was ambitious and anxious to qualify for a post that would enable him to marry Mary Newton. He thought that the detector using very fine magnetized steel wires surrounded by a solenoid in which high-frequency currents reduced the magnetization might make his fortune. Before Guglielmo Marconi, he was able to detect radio waves at a distance of half a mile.

Rutherford developed early an extraordinary ability to recognize, and concentrate upon, the puzzling problems of frontier knowledge in physics. He was never content to follow pedestrian paths of measurement or rounding off of investigations initiated by others. George P. Thomson, in his Rutherford Memorial Lecture, pointed out that Rutherford was working in the Cavendish Laboratory when two completely new physical phenomena were discovered. These were the discoveries of x rays, by Wilhelm Roentgen, and of radioactivity, by Henri Becquerel and each opened up hitherto unsuspected areas of investigation destined to change the course of physics. It is not surprising, therefore, that when J. J. Thomson invited Rutherford to join him in the investigation of the ionization produced in gases by x rays, Rutherford seized the opportunity to move into more exciting fundamental studies.

Rutherford showed that the ionizing effect of x rays was due to the production of positive and negative ions in equal numbers and devised ingenious methods for measuring the velocity of drift of these ions in an electric field. Then in 1898 he investigated the ions produced when ultraviolet light fell on a metal plate, showing that they were all negative ions and that their properties were identical with the ions produced in the gas by x rays. Upon hearing that the radiations discovered by Becquerel to be spontaneously emitted by uranium and thorium were able to ionize gases, Rutherford made observations of the properties of the ions produced, and found them identical with those that he had investigated previously. He showed that two kinds of radiation were present, an easily absorbable and strongly ionizing component which he called "alpha rays," and a much more penetrating radiation to which he gave the name "beta rays." He had found the field of physics in which he was to spend his life.

In August 1898 Rutherford was appointed to a professorship of physics at McGill University. He had applied for the post reluctantly, after assessing his prospects in Cambridge, mostly because of his desire to get married, but, having made the decision he accepted enthusiastically. Upon arrival in Montreal he rapidly established himself, and was soon at work on the further studies of radioactivity that were to establish him as the greatest experimental physicist of his day. In the summer of 1900, he went to New Zealand to collect his bride, returning to McGill in the autumn. In 1901 their only child, a daughter, was born.

Rutherford's subsequent work in Montreal, Manchester, and Cambridge, is part of the history of science, in every textbook.

Lawrence, early years

Lawrence's grandfather, Ole Lawrence, left his home in Norway to settle in Madison, Wisconsin, in 1840. There he became a school teacher in a primitive, pioneering community. He sent his son, Carl, to the University of Wisconsin, from which he graduated in 1894. Carl followed his father's profession as a teacher and showed that he inherited the pioneering spirit, for he moved farther west to South Dakota as a Latin and history master. He became superintendent of public schools in the small community of Canton, and while there, married Gunda Jacobsen, the good-looking daughter of Norwegian immigrants, in 1900. Ernest Lawrence was born to them on 8 Aug. 1901.

Ernest's parents were good people, in the old-fashioned sense of these words. Although his father had a degree in arts, and had taught the humanities, he was not a scholar. The mother, a teacher of mathematics before her marriage, became an excellent wife and mother. She was a strict Lutheran, mingling high principles and loving care in the upbringing of her two sons, Ernest and John. From his parents Ernest acquired a strict moral code and a belief in the inherent decency of most human beings. Carl's ability as an administrator, combined with his integrity, led to his becoming in turn head of the Southern State Teachers' College in Springfield, and then of Northern State Teachers' College in Aberdeen, South Dakota. So, the family enjoyed modest means, but not sufficient to enable the boys to indulge in extravagances without earning money for themselves.

Ernest grew to be a tall, gangling youth. Unlike Rutherford, he did not enjoy the rough and tumble of team games like football but enjoyed tennis, which he played well, if not brilliantly, throughout his life. His career at high school was not outstanding, and though he showed promise in science, he performed indifferently in English. He read very little, and in later life was sarcastic about and impatient of his humanist colleagues, seeing little practical good in their work. He was never a cultured man and had few of the social graces so that he made few friends among girls and did not shine in extracurricular activities of the school. However, he was by no means antisocial, these traits arising from indifference towards any activity that did not fire his interest. He was ambitious and worked hard and consistently, so that he graduated from high school at 16 years of age after three, instead of the usual four years.

During the long summer vacations,

RUTHERFORD AT 21, while a student at Canterbury College, University of New Zealand. Photo from A. S. Eve, *Rutherford*, Cambridge University Press.

LECTURING AT McGILL University, 1907, after Rutherford left Cambridge.

JESSE BEAMS shares a laboratory with Lawrence at Yale University, 1927, where they developed a technique to observe the lifetimes of excited atomic states.

Lawrence worked on farms in the district, as a salesman for aluminum ware and in other ways earned the money required to buy the necessities of an American boy with a mechanical turn of mind—motor cars of various vintages, radio receiving equipment, tools and electrical gadgets, and so on. No doubt under the influence of the concern for others of his parents, he decided upon a career in medicine, and he was sent to a small private college, St. Olaf's in Minnesota, to begin his preliminary studies. He was too young and unsettled to do well there. After a year he moved to the University of South Dakota. He soon applied to the dean, Lewis E. Akely, for permission to build and operate a radio transmitting equipment. Akely was much impressed with the knowledge and ambition of the youth, and persuaded him to turn to physics, providing him with individual tuition in the subject in order to give him a start. After graduation in chemistry—he had not abandoned his ambition to do medicine—Lawrence was persuaded by his close friend, Merle Tuve, and by the offer of a fellowship, to move to Minneapolis. There he worked with W. F. G. Swann, an English immigrant who had been working in geophysics in Washington, but who had joined the University of Minnesota in order to work in more basic physics. Leonard Loeb recalls that Swann was not popular with his colleagues but that he got on extremely well with young graduate students, inspiring them to do research of quality and encouraging them with help and discussion. Under his influence, Lawrence abandoned his desire for a medical career. Swann introduced him to the exciting field of experiment arising from development of the quantum theory. His early interest in electromagnetism was stimulated and developed. He took his master's degree early in 1923, and later that year moved with Swann to Chicago.

In Chicago Lawrence found himself in a very different environment where research was vigorously pursued by an outstanding group of physicists. He was stimulated greatly by contact with Arthur Compton, at the time completing his work on the Compton effect. But he found himself also in a department run on strictly European lines, where the professor was all-powerful and status determined the relationships among members of the laboratory. Neither Swann nor Lawrence was at ease in this atmosphere, and when Swann accepted a post at Yale, a year later, Lawrence went with him. In Chicago Lawrence had learned the real meaning of research, and he threw himself into it with complete devotion. But it was at Yale that his gifts as an experimenter, aided by his energy and enthusiasm, really flowered. For his PhD he worked on the photoelectric effect in potassium vapor, carrying out beautiful experiments that demonstrated clearly that he was a physicist of high quality. Under a National Research Council Scholarship, and after appointment to an assistant professorship, Lawrence continued with his researches. He made precise observations of the ionization potential of mercury vapor, of importance in the determination of the value of Planck's constant h and devised an elegant method of measuring the ratio of charge to mass of the electron. With Jesse Beams, who became his firm friend, he developed a beautiful technique for measuring very short time intervals, which was applied to observations of the lifetimes of excited states of atoms.

In 1928 Lawrence was offered an associate professorship at the University of California, in Berkeley, having turned down an earlier offer of an assistant professorship. A lengthy correspondence with Elmer Hall, the chairman of the physics department, and with Raymond Birge, who had called on Lawrence in Yale and was much attracted by him, has been faithfully recorded by Birge in the history of the department that he is writing. It seems that Lawrence was attracted to California by the opportunity to teach an advanced course and to direct the work of research students, activities reserved in Yale for more senior members of staff. Birge pointed out the good opportunities for rapid advancement of a good man in Berkeley, contrasting this with the policies at Yale, Harvard and Princeton, where it was almost impossible to "get anywhere, after one was there, except under very special circumstances. . . ." Lawrence wrote to Birge saying that some men in Yale were very "sore" that he should even consider a position in California to be comparable with one in Yale. "The Yale ego is really amusing. The idea is too prevalent that Yale brings honor to a man and that a man cannot bring honor to Yale."

Lawrence accepted the offer from Berkeley, and arrived there in August 1928. He set to work at once to continue his work on the photoionization of cesium vapor, used the techniques which he had developed with Beams for the measurement of short time intervals in observations of the early stages of the spark discharge, and one of his research students, Frank Dunnington, developed his method for measuring the charge-to-mass ratio of the electron. He was not committed to this type of investigation, however. He felt that the current challenge in physics was the investigation of the atomic nucleus, rather than of the atom as a whole. He was impressed by the limitations of the methods of investigation developed by Rutherford, who bombarded nuclei with alpha particles emitted by naturally occurring radioactive substances. Like Cockroft, he appreciated Rutherford's desire to be provided with much more intense beams of even more energetic particles with which to probe the internal structure of nuclei.

Lawrence has recorded how, early in 1929, he read a paper by Rolf Wideröe on the use of high-frequency voltages for accelerating charged particles. He recognized that it should be possible to use a magnetic field to curl the paths of such particles into a spiral, and that because the Larmor time-of-revolution in the field was independent of the energy, they could remain in resonance with the voltage across an accelerating gap. Robert Brode has told me of a visit to him by Lawrence the day after seeing the article, enquiring whether the mean free paths of ions could be made long enough for them to suffer negligible scattering by residual gas in their very long spiral paths. Lawrence's colleagues agreed that his calculations were correct, but they were dubious whether the method could be applied in practice.

In 1930, Edlefsen, who had completed his PhD thesis, constructed crude models of the system and observed some resonance effects. Livingston joined Lawrence, after Edlefsen left that summer, and built an improved model that showed resonances corresponding with the rotation times of molecular and atomic ions of hydrogen. By Christmas 1930, a 6-in model surprisingly like a modern cyclotron, was in operation, producing hydrogen ions with energies of 80 000 eV.

The "magnetic-resonance accelerator," as the cyclotron was first named, had become a reality. Lawrence had found his life's work.

In 1932 Lawrence married Molly Blumer, daughter of a distinguished medical man, whom he had met while at Yale and whom he had courted for some years. They had six children, two boys and four girls. He was happy with his family, and the children enriched the life of both. Lawrence appears to have been a normal scientist-father, much preoccupied with his work, alternatively indulgent and too strict, with his serene and capable wife holding the balance and creating the home.

The two compared

The similarity between the early careers of the two men is apparent. The earliest interest of each was in radio. However, while Rutherford abandoned that field completely when he turned to the study of radioactivity, the radio-frequency problems of the cyclotron kept alive the interest of Lawrence. With David Sloan and Livingston he built his own oscillators, and after the war he developed a picture tube for color television that is now manufactured by the Japanese firm, Sony. Each moved from radio into atomic physics, and then to the study of the atomic nucleus. Each was single-minded, working indefatigably towards a goal once it was chosen. Each showed tremendous enthusiasm, which he was able to convey to others.

In his early work, Lawrence showed an insight into physics very like that of Rutherford. Whereas Rutherford continued throughout his life to explore in the frontiers of knowledge, however, Lawrence chose to contribute to physics less directly. After the discovery and successful development of the cyclotron, Lawrence's flair for organization and his business ability enabled him to build the first of the very large laboratories in which massive and expensive equipment was designed, built and used by the able teams of men he attracted to work with him for investigations into basic problems in physics in which he played little part, personally. This pattern of research has become the modern approach all over the world. Rutherford, on the other hand disliked large and expensive equipment. He preferred to remain involved, personally, in almost all the work going on in his laboratory. His interest and ability in administration and finance were rudimentary. He dominated the laboratory by his sheer greatness as a physicist and provided for his colleagues and students only the very minimum of equipment required for an investigation. Rutherford, with his roots in the soil and the hard, practical life of New Zealand, bucolic in appearance, became the deep thinker and the originator of new physical concepts. Lawrence, brought up in an academic atmosphere, impressive and scholarly in appearance, became the originator of new techniques and of the large-scale engineering and team-work approach to discovery.

Both men were extroverts and good "mixers" in company. Donald Cooksey recalls that when Lawrence entered a room filled with great industrialists or successful politicians, his presence was at once noticed, and his impact upon them was profound. Rutherford, however, could be taken for a farmer or shopkeeper, and it was not till he spoke that he was noticed by those who did not know him. Neither was a good speaker or lecturer; yet each influenced and inspired more colleagues and students than any other of his generation. Both built great schools of physics that became peopled with other great men, and Nobel prizes went naturally to members of their laboratories. Each was most generous in giving credit to his junior colleagues, creating thereby extraordinary loyalties.

Rutherford and Lawrence were self-confident, assertive, and at times overbearing, but their stature was such that they could behave in this way with justice, and each was quick to express contrition if he was shown to be wrong.

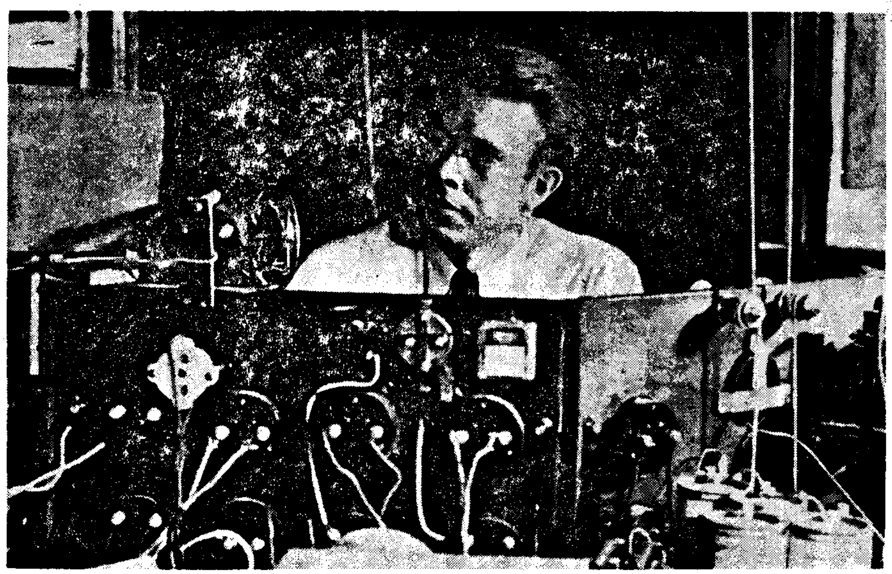
Neither Rutherford nor Lawrence could tolerate laziness or indifference in those who worked with them. Rutherford said to a research student from one of the dominions, at tea before a meeting of the Cavendish Physical Society, "You know, X, I do not believe that you are in and at work because your hat is hanging behind your door!" Such a remark was far more effective than any reprimand. During the hectic days of the Manhattan Project in the war years, Lawrence spoke to me several times of individuals whom he felt did not share his sense of urgency and complete dedication to the task in hand. "I don't know what has gone wrong with Y. He's lazy and his attitude is affecting those round him. I think we'd better get rid of him."

Rutherford had a great and affectionate regard for Niels Bohr, who had worked with him in Manchester. Lawrence could not understand the attitude of the gentle theoretician, who had been smuggled out of Denmark by the British and brought to Los Alamos, where it was thought that his genius could aid the design of a nuclear weapon. While the task was not completed, Lawrence could see no sense in Bohr's worries about how it should be used, or his concern about the part the devastating new weapon could play in the creation of a world without war. Great as was his admiration for the man who had made a living reality of Rutherford's nuclear atom, he felt that Bohr was actually holding back progress and would be better away from the project. On his part, Bohr found it difficult to understand the complete objectivity of Lawrence over an undertaking which created a crisis in human affairs to which men of science could not be indifferent.

Although wholly dedicated to the pursuit of scientific knowledge, both Rutherford and Lawrence delighted in the company of men who had achieved greatness in other spheres. Because of their positions and reputations, they made many contacts and a multitude of friends among industrialists, politicians, lawyers, medical men and the higher echelons of the civil service. They were at home in such company and enjoyed the good living which many such men accepted as part of their existence. But there was one great difference. Rutherford enjoyed what has been called smoking-room humor. Although his own memory for such stories was not good, his great roar of booming laughter was to be heard after dinner as he savored the subtlety of some lewd tale. I never heard Lawrence swear, under any circumstances, and his reaction to off-color humor was not encouraging.

Both Lawrence and Rutherford could be devastatingly blunt and uncompromising when faced with evidence of lack of integrity, or of gullibility, in scientific work. I recollect an occasion when Rutherford was asked to advise whether the inventor of a diagnostic machine, which had been reported upon favorably by one of the Royal physicians, should be paid a large sum of money for rights to use his equipment. Diseases were alleged to be diagnosed by connecting electrodes to the patient and observing the deflections of meters indicating excess or defect of various elements in the patient's body. The inventor explained that the "black box" contained radioactive varieties of each of the elements, whereupon Rutherford became very angry, pouring scorn on both the fraudulent inventor and the gullible physicians who believed in the efficacy of his machine. I am told that Lawrence was invited to examine the claims of a chemist in Berkeley who maintained that isotopes of the chemical elements could be detected, and their proportions measured, in incredibly small concentrations, by observation of certain optical resonances in polarized light, which were characteristic for each individual isotopic mass. Looking through the eyepiece, he could find

RUTHERFORD, IN 1926, visits New Zealand as Cawthron Lecturer.



LAWRENCE AT CONROLS of the 37-in. Berkeley cyclotron, about 1938.

no evidence whatever of the maxima and minima which were said to exist. He burst into laughter, in a cruelly embarrassing manner, at the self-delusion of the young observer, who had been persuaded by the senior perpetrator of the hoax that there was something to observe.

Politics

In politics, Rutherford was what would be called nowadays, a woolly liberal. My wife and I spent many periods with the Rutherfords at their country cottage, "Celyn", in the beautiful Gwynant Valley of North Wales, and later at "Chantry Cottage" in Wiltshire, where the walking was less arduous. He and I often had political arguments, which were particularly hot at the time of the abdication of Edward VIII. I thought that no harm would come if Edward were allowed to marry Mrs Simpson, whereas Rutherford argued that it would do irreparable harm to the monarchy. His main concern was that science should be used properly in the development of the economy, and on one of his rare appearances in the House of Lords, he advocated the establishment of a ministry of prevision to keep the government informed about the advance of science and technology and the probable impact upon industrial development. He was most generous and open-hearted, and did all that he could to aid the victims of Nazi persecution. He was as suspicious of communism as he was of extreme conservatism, but he liked Stanley Baldwin, one of the most conservative prime ministers Britain ever had. At heart, he was apolitical, but when pressed, declared that he was a liberal.

Ernest Lawrence was both an idealist, who cared intensely about the future of his children and all mankind, and a pragmatist, who saw little good in the obsession of some of his colleagues with the examination of social and political schemes for alleviating the lot of humanity. Sometimes during the war, he and I walked up or down the hill between the Radiation Laboratory and the campus of the university. The downward trip usually began by his drinking a carton of cold milk, which I loathed, the liquid portion of which often fertilized one of the stately eucalyptus trees planted on the hillside. We would pause on the way to gaze down over the unforgettable beauty of San Francisco Bay. Then, and while walking, he would tell me of his deep concern that science be used fully to aid the development of the human race, and of his admiration for the practical steps that Franklin Roosevelt was taking to enable this to happen in the United States. He would outline what he could see ahead in the application of physical knowledge in communications, and the productivity of industry and agriculture. He would express his conviction that knowledge of matter and radiation would transform the biological sciences and provide tools for medicine that would alleviate, cure and prevent disease. He felt that this was a task for mankind, and not only for America, and he was anxious to help create a world situation in which all knowledge could be shared by all men. In a practical way he did this whole-heartedly, helping us all, wherever we were, to build cyclotrons, by providing freely drawings, full details, and even his thoughts about improvements upon what had been built in Berkeley. Of course he could not escape entirely the atmosphere of the times, and after the end of the war, he veered somewhat towards a more restricted and less generous view of the part that his great country should play in maintaining the peace and assisting other nations. But this was true only of his politics, and his deep commitment to the defense of America. In his science, he remained the same open-hearted believer in openness and in the value of exchange of knowledge and of information in the removal of international misunderstandings.

However, Lawrence was genuinely apolitical. He had inherited liberal democratic leanings from his parents, but he could not become excited about political issues. For instance, he was quite unaffected by the "loyalty oath," which the university imposed upon members of its staff, and which caused great dissension among some of them. Although unable to appreciate the strong objections of many of his colleagues to what he regarded as a trivial obligation imposed by those who generously supported his laboratory, nevertheless, he fought hard for them as individuals.

Advice on cyclotrons

It is interesting here to recall that the first inquiry Lawrence received from anyone about the possibility of construction of a cyclotron elsewhere, was from Frédéric Joliot, of Paris. On 14 June 1932, he wrote from the Laboratoire Curie, saying that he had read with great interest Lawrence's publication on the production of ions with high velocity. "Votre travail me paraît remarquable, et les études que l'on peut faire avec de tels rayons sont d'un grand intérêt." [Your work seems remarkable to me, and the studies that can be made with such rays are very interesting.] He would like to build an apparatus of a similar type, and to do it rapidly. To this end, he requested two reprints of the article, and any details of construction of the "points les plus délicats" [the most delicate points]. On 20 Aug. Lawrence replied, apologizing for the delay, and told Joliot that he might be able to obtain a magnet made for a Poulson arc radio transmitter, similar to one that Lawrence had obtained in the United States, which he understood was being dismantled at Bordeaux.

The generous attitude of Lawrence towards others desiring to build cyclotrons of their own is well illustrated by the following extract from a letter to Kenneth Bainbridge, dated 6 Feb. 1935:

> "I have just received a letter from Professor [George] Pegram at Columbia, saying that they want to embark upon the construction of a cyclotron provided that I have no objections. I am writing him that, rather than having objections I am more than delighted that they are planning to build a cyclotron. The cyclotron to my mind is by far the best ion accelerator for nearly all nuclear work, and it would give me a great deal of pleasure if many laboratories would build them."

On 27 Nov. 1935 Lawrence wrote to Chadwick, congratulating him on the award of a Nobel Prize, and offering to give him every help in building a magnetic-resonance accelerator in Liverpool. He said that the Cavendish must miss Chadwick greatly, but that this was compensated by the fact that he would build in Liverpool another great center of nuclear physics. Chadwick replied that he felt rather lucky to get a Nobel Prize and thanked Lawrence for his offer to help to build "your magnetic-resonance accelerator, which ranks with the expansion chamber as the most beautiful piece of apparatus I know." In letters about the construction of cyclotrons by others, Lawrence always emphasized that, contrary to the ideas of many, the cyclotron was not a difficult piece of equipment to get into operation.

The word "cyclotron" did not appear in any publication from the Radiation Laboratory till 1935, in a paper by Lawrence, Edwin M. McMillan and Robert Thornton,[1] where the following footnote is inserted:

> "Since we shall have many occasions in the future to refer to this apparatus, we feel that it should have a name. The term 'magnetic-resonance accelerator' is suggested. . . . The word 'cyclotron,' of obvious derivation, has come to be used as a sort of laboratory slang for the magnetic device."

Running their laboratories

The Cavendish Laboratory, under Rutherford and his predecessors, was always short of money. Rutherford had no flair and no inclination for raising funds. Only under extreme pressure, first from the ebullient Peter Kapitza, and later from Cockcroft and me, was he prepared to fight hard for money for large or complex equipment. He never sought riches and died a comparatively poor man. Lawrence, on the other hand, had shrewd business sense and was adept at raising funds for the work of his laboratory. Apart from his early interest in medicine, he realized early the medical possibilities of the radiations produced by the cyclotron, and did not hesitate to use these in his search for funds. In 1935 he wrote to Bohr:

> "In addition to the nuclear investigations, we are carrying on investigations of the biological effects of the neutrons and various radioactive substances and are finding interesting things in this direction. I must confess that one reason we have undertaken this biological work is that we thereby have been able to get financial support for all of the work in the laboratory. As you well know, it is so much easier to get funds for medical research."

Similarly, after the war, he made full use of the wartime achievements of the Radiation Laboratory in raising the support required for the very large expansion of its activities. However, it was his concern for the defense of his country and his belief that it was unwise to confine the development of nuclear weapons to Los Alamos, which led him to establish a branch of the laboratory devoted to this work at Livermore.

Lawrence's phenomenal success in raising money for his laboratory was undoubtedly due to his able handling of executives in both industry and government instrumentalities. His direct approach, his self-confidence, the quality and high achievement of his colleagues, and the great momentum of the researchers under his direction bred confidence in those from whom the money came. His judgment was good, both of men and of the projects they wished to undertake, and he showed a rare ability to utilize to the full the diverse skills and experience of the various members of his staff. He became the prototype of the director of the large modern laboratory, the costs of which rose to undreamt of magnitude, his managerial skill resulting in dividends of important scientific knowledge fully justifying the expenditure. But in achieving this, he had to give up personal participation in research. His influence on the laboratory programs remained profound, and his enthusiasm radiated into every corner of the institution. William Brobeck, who joined the Radiation Laboratory in 1936 as an engineer, recalls that Lawrence took an animated part in all discussions of technique and showed an extraordinary ability to see a piece of equipment as a whole, avoiding becoming bogged down in detail. Lawrence was a regular visitor to each section of the laboratory until illness caused him to appear very seldom outside his office.

Rutherford's method of running a laboratory was in striking contrast to that of Lawrence. He was not much interested in the apparatus for its own sake, believing that techniques grew from the demands of the experiment. Like Lawrence, he advocated a simple, preliminary approach, a sort of skirmish into the territory to be explored, followed by refinement if the reconnoiter showed promise. He would roam round the laboratory, discussing results and the physical knowledge they revealed, rather than apparatus. His stimulus was enormous, and his influence direct. A glance at any list of publications from the Cavendish Laboratory, or from the laboratories in McGill or Manchester in his periods there, reveals how deep was his influence on the researches carried out. Lawrence worked to give others the opportunity to achieve important results; Rutherford was so great a physicist that almost every member of his laboratory found himself working upon some problem that Rutherford had suggested, or that arose directly from Rutherford's own work. This dominance was not imposed upon his colleagues and students. They often began work along lines of their own choosing, but rapidly found that the instinct of Rutherford's genius was a surer guide to interesting and important results.

Both Rutherford and Lawrence gave coherence to laboratories inhabited by workers of differing temperaments and varying abilities. Under their influence, each gave of his best; all rejoiced in the outstanding achievement of one of their number, and each felt himself to be part of the whole, sharing its triumphs and its vicissitudes.

Seventh Solvay Congress

Although Lawrence had made a very rapid tour of Europe with his friend Beams in the summer of 1927, he and Rutherford did not meet till 1933. In that year, the Seventh Solvay Conference, held in Brussels from 22 to 29 Oct., was devoted to nuclear physics, and, naturally, Lawrence was invited to attend. He was eager to go, since this would give him the opportunity to meet the principal workers in his field. Those taking part included:

From Cavendish Laboratory:

Ernest Rutherford
James Chadwick

John Cockcroft
Patrick Blackett
Paul Dirac
Cecil Ellis
Rudolf Peierls
Ernest Walton

From Institut du Radium, Paris:
Marie Curie
Irene Joliot-Curie
Frédéric Joliot
M. S. Rosenblum

From the Physical Institute, Leipzig:
Werner Heisenberg
Peter Debye

From elsewhere:
Neils Bohr (Institute of Theoretical Physics, Copenhagen)
Albert Einstein (then living in Belgium)
Erwin Schrödinger (Physical Institute, University of Berlin)
Wolfgang Pauli (Physical Institute, Zurich)
Louis de Broglie (France)
Marcel de Broglie (France)
Enrico Fermi (Physical Institute, University of Rome)
George Gamow (Institute of Mathematical Physics, Leningrad)
Abraham Joffe (University of Physics and Mechanics, Leningrad)
Walther Bothe (Physical Institute, University of Heidelberg)
Lise Meitner (Kaiser Wilhelm Institute, Berlin)
Francis Perrin (Institute of Chemistry and Physics, Paris)
Léon Rosenfeld (Institute of Physics, University of Liège)
H. A. Kramers (Institute of Physics, University of Utrecht)
Nevill Mott (University of Bristol)

Ernest Lawrence, the only American invited, naturally was greatly pleased to find himself among this group of eminent physicists who, together, represented almost all that was then known, from experimental and theoretical investigation, of the atomic nucleus. His invitation from the President, Paul Langevin, asked him to participate in "l'examen de questions relatives à la constitution de la matière" [the examination of questions relative to the constitution of matter], and reports were to be read by Rutherford, Chadwick, Bohr, Heisenberg, Gamow, Cockcroft, and M and Mme Joliot. It was clearly to be an exciting meeting, as it was only a year earlier that the neutron had been discovered, and transmutation of nuclei by artifically accelerated beams of charged particles had been achieved.

In a letter to Langevin, dated 4 Oct 1933, written after he had read the papers that had been circulated to those invited, Lawrence stated that he wanted particularly to make some rather extensive observations on Cockcroft's report, and that he might wish to comment on papers by Chadwick, Joliot, and possibly Gamow. He was able to obtain funds to meet the costs of his trip, but owing to his commitments in Berkeley, he could stay in Europe for only a very limited period.

At this time, Lawrence and his coworkers had used the cyclotron to confirm the results of Cockcroft and Walton on the disintegration of lithium by proton bombardment, and had extended their observations on this and other transformations to higher energies. Lawrence had eagerly availed himself of the opportunity offered by the success of Gilbert N. Lewis, at Berkeley, in producing almost pure samples of heavy water, and had accelerated the nuclei of the new hydrogen isotope in the cyclotron. His team observed an enormous emission of protons and neutrons from every target that was bombarded, and this similarity of results, irrespective of target material, had led Lawrence to put forward the hypothesis that the nucleus of heavy hydrogen, called the "deuton" by Lewis, was unstable, breaking up in nuclear collisions into a proton and neutron. Meanwhile, Lewis had presented samples of heavy water to many investigators, including Rutherford, and we had been making observations in the Cavendish Laboratory that were not in accord with Lawrence's view that the deuton was unstable.

Lawrence went to the Solvay Conference prepared to defend his hypothesis and to back the cyclotron as the type of accelerator most versatile for experimental work in nuclear physics. The marginal notes made by him on the copies of the reports presented, give interesting information about his attitudes. Some of these are vigorous, as the large cross over Cockcroft's assertions that "only small currents are possible" from the cyclotron, and when Cockcroft restated this

WATSON DAVIS, SCIENCE SERVICE

CYCLOTRON MODEL is held by Lawrence in 1930, year after conception.

later, he wrote, "Not true," boldly in the margin. In several places he complained that the deuton-breakup hypothesis received no mention, and it becomes clear that he did not appreciate fully the calculations of neutron mass given by Chadwick, or the observations of Cockcroft, and of Rutherford and me, which were not in accord with his idea. He showed particular interest in those observations reported by the Joliots on gamma rays produced from atoms bombarded by alpha particles, both those collisions that result in capture of the alpha particle, and those in which a nucleus is excited, without actual capture.

Lawrence's meticulous care to give credit to his colleagures for their part in the work in his laboratory is evident from his insistence upon the addition of their names—Malcolm Henderson, Milton White, Sloan, Lewis and Livingston—wherever Cockcroft's paper mentioned only Lawrence.

Chadwick recalls, in a letter to me, that Rutherford was much impressed by the vigorous young Lawrence, and remarked to Chadwick, "He is just like I was at his age."

Lawrence paid a brief visit to the

Cavendish Laboratory after the Solvay Conference, and it was then that I met him. We had a vigorous discussion, with Lawrence sticking firmly to his concept of an unstable deuton. When he had gone, Rutherford, said, "He's a brash young man, but he'll learn!"

Cooksey tells me that he met Lawrence at the boat in New York on his return to America. Lawrence was bubbling over with enthusiasm for all that he had seen and learned. He was particularly enthusiastic about the great power of the neutron as an agent for disintegrating nuclei, and expressed the view that, before long, these would make possible a self-propagating reaction, and hence the practical release of energy from nuclei. A truly prophetic remark.

Deuton instability

After his return from the Solvay Conference, Lawrence wrote to Cockcroft informing him that, with Livingston and Henderson, he would concentrate upon the origin of the protons, with a range in air of about 18 cm, which were emitted from all targets bombarded with deutons. Firstly, they would try to clear up the uncertainty about contamination of the targets, and if this did not turn out to be the source of the particles, they would "continue the experiments to shed further light on the origin of the 18 cm protons." He reported also that, on his way back, he had visited Washington, where Tuve had a beam of protons with an energy of 1.5 MeV from his Van de Graaff accelerator.

"I persuaded Tuve to investigate the origin of the 18 cm protons and the hypothesis of the disintegration of the deuton right away. I want to get the matter cleared up as soon as possible and it will be a great help if Tuve, with his independent set-up, will investigate the problem."

He wrote also to Gamow on 4 Dec. 1933, saying that he had been paying particular attention to the hypothesis of the disintegration of the deuton, using clean targets and carefully purified materials. "However, we find that the yield of protons and neutrons produced by the bombarding deutons is quite independent of our endeavors to clean the targets." They found that 2.8-MeV deutons produced disintegration protons in the same proportions as observed at 1.2 MeV. On 28 Dec. 1933 he wrote again to Gamow:

"The experimental evidence that the deuton disintegrates is growing. Lately, we have observed the emission of long range protons (up to about 20 cms) resulting from the bombardment by protons of targets containing heavy hydrogen. Though perhaps the matter cannot be regarded as entirely settled yet . . . certainly it must be admitted that the evidence is preponderantly in favor of the hypothesis of the energetic instability of the deuton."

Cockcroft, in a letter to Lawrence of 21 Dec. 1933, reported further work on the long range protons produced by bombardment with deutons from lithium, carbon and boron, and noted that while iron gave a small yield of protons, none were observed from copper, gold or copper oxide.

"We have so far not worked beyond 600 kV, and it may well be that some groups appear at higher voltages. I feel myself, however, that the evidence so far is against your interpretation of the break up of H^2."

Lawrence replied on 12 Jan. 1934: "It seems to me that you are hardly justified in feeling that the evidence obtained by you so far is against the interpretation of the break-up of the deuton, since you have not worked at voltages above 600 kV . . . it seemed pretty evident from our first preliminary observations that the yield of the group of protons which we ascribe to deuton disintegration is in all cases very small below eight or nine hundred thousand volts. Despite your greater intensities, on the basis of our observations we would hardly expect that you would observe the disintegration of the deuton at the voltage you have been using. . . . I hope that you will soon raise your voltage to eight or nine hundred thousand. Meanwhile I have written Tuve your results and asked him to look into the matter, as I understand he is able to work now above a million volts. I am anxious that the hypothesis of deuton disintegration will be settled to everyone's satisfaction, and to that end it seems essential that independent experiments be carried out in another laboratory."

Cockcroft wrote again on 28 Feb. 1934:

"We have been working steadily on the question of disintegrations by heavy hydrogen. In addition to the results on lithium I reported to you in my last letter, we find three groups of protons from boron. . . . We have been investigating copper, copper oxide, iron, iron oxide, tungsten and silver, with stronger heavy hydrogen, and we find from all of these we get three groups of particles of identically the same range. The first is an alpha particle group having a maximum range of 3.5 cm, the second is a proton group of about 7 cm, and the third is a proton group of about 13 cm. This latter group is the one which you ascribe to the break up of the deuton. It seems in the first place clear that these three groups cannot all be due to this break up, and we therefore feel strongly that the alpha particle group and the 7 cm proton group are at any rate due to an impurity which is probably oxygen. We are not yet certain about the 13 cm group, but are carrying out experiments with white hot tungsten targets which I hope may finally dispose of this possibility. We can observe all these groups at voltages as low as 200,000, and the voltage variation shows the standard Gamow tail to the curve. . . .

"I feel, however, that we have still very good justification for refusing to commit ourselves to your hypothesis of the deuton break up until further experimental work has been carried out."

To this typewritten letter, Cockcroft added the following handwritten postscript:

"We have now found that on boiling in caustic and cleaning thoroughly the 13 cm group is reduced by a factor 10; on heating to 2,600 by a further factor. The 2.5 and 7 cm groups disappear on heating and reappear on oxidation and seem due to oxygen. . . . Oliphant is getting queer results with $H^2 + H^2$."

Lawrence replied on 14 March 1934,

agreeing that Cockcroft's observation that boiling tungsten in caustic reduces the 13-cm group by a factor 10 showed clearly that this is due to a contamination.

"I think it is quite possible that the effects we observed when bombarding targets of heavy hydrogen with hydrogen molecular beams were due, as [C. C.] Lauritsen suggested, to an increase in deuton contamination resulting from partial decomposition of the targets. I cannot understand my stupidity in not recognizing this possibility when the experiments were in progress. Needless to say, I feel there is now little evidence in support of the hypothesis of deuton instability. . . .

"Rather than continuing with preliminary and exploratory experiments at higher voltages, we have decided to embark on careful investigations of the nuclear effects brought to light and we shall make as precise and trustworthy measurements as we can. These recent experiences have impressed upon us forcibly the fact that much of our work has been of too preliminary character to be of value. I regret very much that the question of deuton instability involved you in so much work, and I want to thank you very much for stepping in and clearing the matter up so effectively and so promptly."

Lawrence and his colleagues were relatively new to nuclear physics, and it is not at all surprising that they made mistakes in interpretation of a complex phenomenon. It was characteristic of the young Lawrence that he held tenaciously to his concept of deuton instability, but that when presented with definite evidence that it was wrong, he immediately set to work to change the approach of his team to its experiments in such a way as to avoid similar pitfalls in the future.

Deuton stable after all

Meanwhile, the explanation of the origin of the proton group that had led Lawrence astray had been found in the Cavendish Laboratory. On 13 March 1934, Rutherford wrote to Lawrence:

"I have to thank you for the very interesting letter you sent me some time ago giving an account of your work. The whole subject is certainly in an interesting stage of development and reminds me very much of my early 'radioactivity' days before the theory of transformations cleared things up.

"I think you have heard from Cockcroft about some of our observations the last few months. Oliphant and I have been particularly interested in the bombardment of D with D ions, and I am enclosing a note from Oliphant giving an account of our results. I personally believe that there can be little doubt of the reaction in which the hydrogen isotope of mass 3 is produced, for the evidence from all sides is in accord with it. The evidence for the helium isotope of mass 3 is of course at present somewhat uncertain but it looks to me not unlikely.

"You will see that Oliphant like myself is inclined to believe that the proton group which you observe for so many elements arises from the reaction I have mentioned. We have made a large number of observations with beryllium and other elements but the results are not easy of interpretation. We think the information we have found about the D-D reaction will be helpful in disentangling the data. As you no doubt appreciate, it takes a lot of work to make a reasonably complete analysis of the groups of particles from any element and then it has to be done all over again with the other compounds to try and fix the origin of the groups. There is an enormous amount of work that will have to be done with the lighter elements to be sure we are on firm ground.

"You will have seen about Cockcroft's results due to the bombardment of carbon by protons. This no doubt produces the radio-nitrogen of the Joliots but we can obtain quite strong sources of positrons by this method. I heard that Lauritsen or yourself had observed similar effects with D bombardment. The whole subject is opening up in fine style. You will also have seen that Oliphant and Co have separated the lithium isotopes and confirmed the tentative conclusions we put forward before." My note went as follows:

"You may have heard of the experiments which we have carried out during the last week or two on the effects observed when heavy hydrogen is used to bombard heavy hydrogen. As I believe these are intimately related to your own work, I should like to tell you what we have found."

The letter went on to give details of the results, and of their interpretation as due to two competing reactions, the first leading to the production of hydrogen of mass 3 and a proton, with ranges of 1.6 cm and 14.3 cm respectively, and the second to helium of mass 3 and a neutron.

"We suggest, very tentatively, that your results may be explained as due to the bombardment of films of D and of D compounds. Our results with C, Be, etc., could all be accounted for by the presence of less than one monomolecular layer of D. . . ."

On 4 June 1934 Lawrence replied to my note, saying that the late answer was due to his desire to be able to send some news of interest.

"Your experiments on diplons, together with Cockcroft and Walton's recent work, have certainly cleared things up in beautiful fashion. There can no longer be any doubt that our observations which we ascribed to diplon break-up, are in fact the results of reactions of diplons with each other."

He ended his letter with a reference to Cockcroft's contention, in his Solvay Conference paper, that the cyclotron gave only small currents:

"Dr. Cockcroft might be interested to know also that we are gradually increasing our currents of high velocity ions, and that now we are working regularly with more than a microampere of either 3 MV diplons or 1.6 MV protons and several microamperes of 3 MV hydrogen molecule ions."

Lawrence had already replied to Rutherford's letter on 10 May 1934, saying:

"I want to thank you for your very much appreciated letter. Everyone here was delighted to learn of the extraordinarily interesting experiments you have been doing on the reactions of D-ions with each other (perhaps I should say diplons. I do appreciate the force of your arguments in support of diplon,* but all of us here have become quite accustomed to deuton and it would be some effort to change).

"It is difficult for me to understand how we could have failed to detect the effect of diplons on each other. We did notice about twice as many long range protons from the heavy hydrogen target under bombardment by diplons, but the difference between the targets was much greater under proton bombardment. The fact that the calcium hydroxide targets decompose readily may in some way account for our observations. Professor Lewis has prepared some ammonium chloride targets and we shall investigate the matter soon.

"The manuscript of Cockcroft and Walton's admirable paper has just arrived. There can hardly be any doubt any longer that most of the effects which we ascribe to disintegration of diplons are in fact due largely to a general contamination of heavy hydrogen in our apparatus. I certainly appreciate the manner in which this complexity of nuclear phenomena already brought to light makes it clear that it is easy to fall into error, and that a good deal of cautious work must be done for trustworthy conclusions.

"Fermi's observation of radio-activity induced by neutron bombardment is a case in point. When we bombard various targets with three million volt deutons, large numbers of neutrons are always produced, which among other things produce the types of radio-activity discovered by Fermi. On receiving Fermi's reprint announcing the effect, we looked for it and found that it was no small effect at all. For example, we found that a piece of silver placed outside of the vacuum chamber about three centimeters from a beryllium target bombarded by a half micro-ampere of three million volt deutons became in the course of several minutes radio-active enough to give more than a thousand counts per minute when the silver piece was placed near a Geiger counter. We are now studying this type of radio-activity induced in various substances and will not return to the effects produced by diplon and proton bombardment until we understand pretty well the neutron effects.

"Dr. [Franz] Kurie has been photographing with the Wilson chamber the recoil nuclei and disintegrations in oxygen produced by neutrons from beryllium bombarded by deutons. Although the Wilson chamber is about twenty inches from the neutron source and therefore subtends a rather small solid angle, the neutron intensity is sufficiently great to give him something like five or ten recoil oxygen nuclei in each picture and about one disintegration fork per ten pictures. Most of the disintegrations appear to result in C^{13} and an alpha-particle, but Kurie has a dozen or so which seem to involve the emission of a proton and therefore the formation of N^{16}. But these conclusions are highly tentative. At the moment Kurie is busy making measurements on his photographs.

"We have sent off for publication a manuscript on the transmutation of fluorine by proton bombardment and I am enclosing the essential curves of the experimental results. As far as we can determine, the alpha-particles from fluorine have a range of between six and seven centimeters, depending on the energy of the bombarding proton. These results support the possibility suggested in your paper that the 4.1 cm alpha-particles observed by you are due to boron.

"Dr. McMillan has been studying gamma radiation from various substances and finds among other things that fluorine emits under proton bombardment, a five million volt monochromatic gamma radiation of considerable intensity. Some day perhaps a short range group of alpha particles from fluorine will be found to account for this gamma radiation.

"But possibly the most interesting result that McMillan has found about this radiation is its absorption coefficient. He finds that the absorption per electron of the five million volt gamma radiation varies approximately linearly with atomic number, reaching a value for lead double that for oxygen. In other words, nuclear absorption (pair production presumably) is so great that in going from two and a half to five million volts the absorption coefficient in lead does not decrease a great deal.

"I am glad to hear that you are very well. You need not have told me that you are kept very busy in the laboratory, but I was very glad to hear that the government has given you a substantial grant of money for research and that you are responsible for its disbursement. Also your comparison of your early radioactivity days with the present is very much appreciated. I remember in the course of my graduate studies what a 'kick' I got out of reading of the early work on radio-activity, but I did not even hope at that time that I would have the opportunity to work in a similarly interesting new field of investigation. . . .

"Please tell Dr. Oliphant that I appreciated his letter very much and that I will be writing him directly before long."

Rutherford's brash young man learned very quickly, as Rutherford predicted he would. From that time onward, the contributions made to nuclear physics in the Radiation Laboratory were above reproach and of rapidly increasing importance, as the energy and intensity of beams available from the cyclotron increased. □

(This is the first of two articles on Ernest Rutherford and Ernest Lawrence. The second will appear in the next issue.)

* The evident confusion in nomenclature arose in this way. G. N. Lewis had proposed the name "deuton" for the nucleus of the atom of heavy hydrogen. Rutherford objected strongly to this, feeling that it would inevitably lead to confusion with neutron, especially in the spoken word. After discussion with his classical colleagues, he proposed the name "diplon," for the nucleus, and 'diplogen' for the atom, terms derived from Greek, and analogous to proton and hydrogen. The dual nomenclature was given up eventually, and the compromise "deuteron" and "deuterium" was accepted. It was said by one cynic that Ernest Rutherford was happy when his initials were inserted into deuton!

Reference

1. E. L. Lawrence, E. M. McMillan, R. L. Thornton, Phys. Rev. 48, 493 (1935).

The Two Ernests—II

Sir Mark continues his personal recollections of Ernest Rutherford and Ernest Lawrence. By 1935 precise mass determinations with nuclear reactions were being made at Cavendish. In the following years Rutherford was arranging for new facilities at the laboratory. Meanwhile Lawrence began to use the cyclotron for medical research, learned to extract a beam from the accelerator and found a lot of unexpected radiation. Two years after Rutherford's death, the discovery of fission opened a new era.

by Mark L. Oliphant

BOTH ERNEST RUTHERFORD and Ernest Lawrence led great laboratories and inspired the physicists who worked in them. Rutherford was personally involved in almost all of the work at the Cavendish Laboratory, dominating the laboratory by his sheer greatness as a physicist and providing for his colleagues only the barest minimum of equipment. Lawrence, on the other hand, created at the Radiation Laboratory, the first of the very large laboratories in which massive and expensive equipment was designed, built and used for investigations into basic problems in physics in which he played little part, personally. After the discovery and successful development of the cyclotron at his laboratory, Lawrence enthusiastically offered his assistance in the construction of cyclotrons at laboratories elsewhere.

The two men did not meet until the Seventh Solvay Congress, October 1933. At the meeting, Lawrence defended his hypothesis that the "deuton" (deuteron) was unstable, breaking up in nuclear collisions into a proton and neutron. By May of the following year, however, Lawrence was convinced by experiments in the Cavendish Laboratory that what he had actually observed were reactions of deuterons with deuterons. From that time onward, the contributions of Lawrence's laboratory were above reproach and of rapidly increasing importance as the energy and intensity of the beams available from the cyclotron increased.

Accurate mass measurements

One of the early results of more accurate observations of the energies released in nuclear reactions involving the light elements was realization that the relative masses of the atoms, as given by the mass spectrograph, were not sufficiently reliable to give consistent agreement. In the Cavendish Laboratory, we naturally used the mass determinations made there by Francis W. Aston, whose improved mass spectrometer was then in operation. We came to the conclusion that there was an appreciable error in Aston's value for the mass ratio of hydrogen to helium, a basic determination upon which many of his other mass values depended. Aston was a touchy person and reacted with characteristic violence to the suggestion that there were systematic errors in his list of isotopic masses. On 4 May 1935, Rutherford wrote to Lawrence:

"You will no doubt have heard from Cockcroft and others about what is going on here. We have given a complete account of our beryllium results in the P.R.S. [*Proceedings of the Royal Society*] which appears this month, and you will see that we have put forward a scheme of masses to fit in—practically along the same lines that [Hans] Bethe has independently suggested in your country. At first, Aston took a high line about the accuracy of his results, and the impossibility of any serious error between helium and oxygen, but when I told

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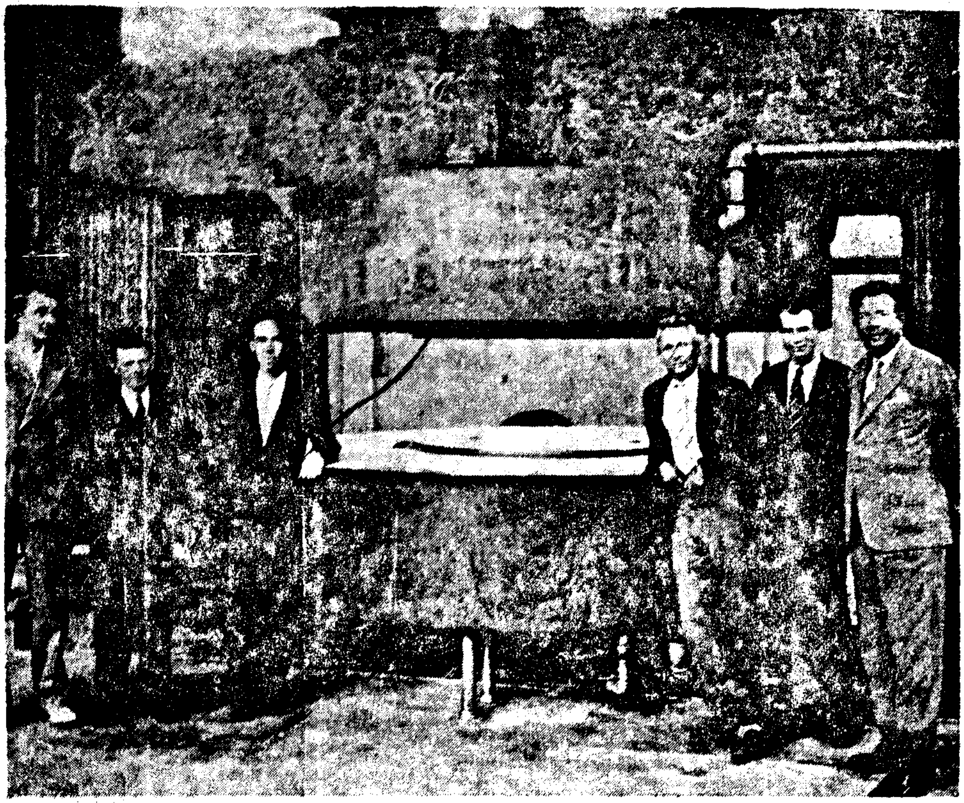
Sir Mark was assistant director of research at Cavendish until 1937, when he became director of the physics department at Univ. of Birmingham. In 1950 he became director of the Research School of Physical Sciences, Australian National University, Canberra. He served for three years as president of the Australian Academy of Sciences.



KEY FIGURES in development and early use of the 60-in. Crocker cyclotron stand beside the machine during construction. Only the magnet yoke and the coils have been completed. Left to right: Luis Alvarez, William Coolidge (who was visiting), William Brobeck, Donald Cooksey, Edwin McMillan and Ernest Lawrence.

struction of an improved design. Perhaps the most interesting result is that the focusing action of the electric and magnetic fields is so nearly perfect that we can get just as large current of deuterons at 4.5 MV as at 2.5 MV. At the present time the apparatus delivers several microamperes of deuterons having a range of 16.7 centimeters (about 4.5 MV). We have bombarded several substances, using these energetic deuterons, and it appears that almost the whole periodic table can be activated, the type of nuclear reaction involved being that in which the neutron of the deuteron is captured by the bombarded nucleus. We have found that gold can be activated in this way, a result which is very surprising. We shall do a good deal more work yet on these things before we can have confidence in the experimental results and theoretical interpretations.

"We were all very much surprised to hear that Chadwick is leaving you to be professor at Liverpool. I suppose it is a promotion for him, but I am sure that if I were he I would be very loathe to leave you and the Cavendish Laboratory."

him that if he did not get to work, I was going to put forward the correct mass scheme, he rapidly started in, and found that he had dropped one or two bricks of reasonable magnitude! I am not quite sure he is right yet, but no doubt he may amend his results later. As a matter of fact, it is obviously very difficult for mass-spectrographic methods to give the same accuracy as from transformations when we are sure of the reaction."

In his reply, Lawrence wrote:

"Your very much appreciated letter was forwarded to me in New Haven, Connecticut, late in May: I was in the East about two months, engaged in my annual task of raising money for the support of our work in the radiation laboratory. I rather expected considerable difficulty in raising needed funds this year, and indeed was rather worried that we might have to restrict our work a great deal, but fortunately matters turned out otherwise. In this country medical research receives generous support, and it was the possible medical applications of the artificial radioactive substances and neutron radiation that made it possible for me to obtain adequate financial support. We are now able to produce several millicuries activity of radiosodium. We are devoting a good deal of attention to the further development of the magnetic resonance accelerator for considerably larger currents and also higher voltages. It is reasonable to expect that it will not be very long before we will be producing ten times as much radioactive substance as at present. However, according to the medical people, at the present time we can provide enough radiosodium for beginning clinical investigations, and we have agreed to begin supplying the University Hospital here early this fall.

"We have lately been making various tests of the performance of our apparatus with a view to the construction of an improved design.

Cyclotrons for medical research

This letter mentions again Lawrence's readiness to develop the medical applications of the cyclotron and its products in order to obtain the funds required for the work of his laboratory. However, his interest in possible medical applications was not only financial. His early ambition to become a doctor and the fact that his younger brother, John, had qualified in medicine and had become an instructor at Yale Medical School had kept his genuine interest in the healing art. In the summer of 1935, John, who had broken his leg, went to California to stay with Ernest while he recuperated. He did some experiments while there, with the aid of Paul Aebersold, a young colleague of Ernest. They exposed rats to neutrons and gamma rays from the cyclotron. On 13 Aug. 1935 Lawrence wrote a letter to Rutherford that I quote in full:

"Dear Professor Rutherford:

"I am very, very grateful to you for the photograph of yourself which

I shall always treasure very highly. In asking Cockcroft to get a photograph of yourself for me and ask you to autograph it, I had in mind that he could purchase one in a bookstore and perhaps persuade you to write your signature on it. I appreciate very much your kindness in sending me the portrait.

"Work is going along quite satisfactorily in our laboratory, although at the moment we are bothered with cathode ray punctures of the insulators of the magnetic resonance accelerator, the result of increasing the voltage and current output. My brother, who is on the faculty of the Yale Medical School, is vacationing here, and I persuaded him to undertake a preliminary investigation of the biological effect of neutrons. He has been exposing rats to neutrons for periods of time from ten minutes to three hours, and has been observing the changes produced in the blood of the rats. The first rat was exposed for a period of three hours, and as a result died, and subsequent experiments indicate that neutron rays are considerably more lethal biologically than x rays. The immediate result is that we are taking rather greater precautions in the matter of exposing ourselves in the course of our work in the laboratory.

"I am very glad to hear that you are well, and again I want to thank you ever so much for your picture.

"With best wishes and highest personal esteem, I am

Respectfully yours,"

John tells me that in fact the rat died of suffocation, being too completely confined! However, an important result was that much more stringent precautions against neutron and gamma radiation were then instituted in the Radiation Laboratory. From then till 1937, John Lawrence visited Berkeley regularly, at intervals of about three months, taking with him biological experiments to be carried out with the aid of the cyclotron. In 1937 he moved to Berkeley permanently to take charge of the medical work with a 60-in. cyclotron provided through the generosity of Crocker. Direct treatment of patients with the neutron beam from the cyclotron began in 1938, in collaboration with Robert Stone of the University of California Medical School in San Francisco. Lawrence had encouraged Sloan to design, and get into operation, an x-ray equipment for about 1 MV, using a resonant transformer in a vacuum, and Stone was using this in the hospital. The mother of Ernest and John was treated for a malignant growth with this equipment by Stone in 1937, and the treatment was so successful that it reinforced the faith of the brothers in the possibility of developing still more effective uses of radiation in the treatment of cancer.

New equipment at Cavendish

A letter from Rutherford to Lawrence, of 22 Feb. 1936, contains the following passages:

"I was delighted to get your letter and to hear how your work is going on. I congratulate you on your success with your apparatus in getting high voltages and intense beams. The neutron photographs you sent me were certainly very impressive, and I can roughly estimate the strength of your artificial source of neutrons in terms of radium emanation.

"I was exceedingly interested to hear also that you [this work was done by John Livingood, under Lawrence's general direction] have been successful in producing radium E from bismuth—a great triumph for the new apparatus. I have a personal interest in this artificial product; for I do not know whether you know that I worked out the changes radium D-E-F long ago in Montreal, and showed that as the β rays decayed an α-ray product grew. The apparatus I used is now preserved in the Physical Laboratory in McGill. I shall be interested to hear the details of your experiments and how much radium E you manage to produce.

"I note what you say about the present stage of your apparatus. At present we are very busy transferring the apparatus from the Royal Society Mond Laboratory, and getting duplicates, and keeping the cryogenic work going as usual. We do not intend to get a duplicate of the big generator for producing strong magnetic fields, but have in view instead the installation of a large magnet for general purposes, and also probably for use as a cyclotron. We have not had time as yet to go into the matter, but I think probably Cockcroft will be writing to you soon to see whether you can give him any information of the best design of magnet to be used for the latter purpose.

"At present we are just beginning the new building for our high tension D.C. plant, and we hope with luck to reach 2 million volts positive and negative, and possibly higher, but no doubt we will find plenty of trouble before it is in working operation. We shall, of course, build up the component parts of the apparatus ourselves so as to keep down the expense.

"Aston will shortly be publishing the new values of the masses of the light elements obtained with his improved spectrograph, and these new values fit in very satisfactorily with transformation data, so that difficulty is removed. I have also heard from several sources that Bainbridge has also done very much the same thing with his new spectrograph, and it will be interesting to see how far these two independent sets of measurements agree. It will be an ultimate test of the accuracy of these two systems."

The reference to the Royal Society Mond Laboratory concerns equipment that had been provided for the work of Peter Kapitza, the Russian engineer-physicist who had joined the Cavendish Laboratory in 1921. He was in the habit of visiting Russia during the summer to see his old mother. In 1935 the Soviet government refused to allow him to return to Cambridge, but offered to buy his equipment from the university in order that he might continue his researches in Russia. With the able help of Cockcroft and others, Rutherford proved himself a better man of business than expected, and negotiated a good price for the equipment. Meanwhile, Rutherford's resistance to the idea of as complex a piece of apparatus as a cyclotron in the Cavendish Laboratory had been worn down, and he was willing to devote part of the sum received from Russia to the acquisition of a large magnet which could be used, inter alia, for a cyclotron.

The reply by Lawrence was characteristic of his generosity towards all who wished to build a cyclotron:

"Thank you ever so much for your good letter. I should have known that you were responsible for the radium D-E-F, but I must confess that I didn't. As regards the yields of radium E by bombarding bismuth with five-million-volt deuterons, I must say that they are quite small. If I remember correctly, several hours bombardment with several microamperes gives, after a few weeks, something like thirty alpha-particles count per minute when the bismuth target is placed near the ionization chamber of the linear amplifier. Measurements on the range distribution of the alpha particles from the bismuth indicate that the transmutation function is exceedingly steep (for nearly all of the alpha particles have very near the full polonium alpha-particle range). It is probable, therefore, that at six million volts, which is the voltage we are now using, the radium E and polonium yield should be very much greater; and doubtless in the near future Dr. Livingood will continue experiments at this higher voltage.

"We have recently made some alterations of the cyclotron which have made it possible to withdraw the beam completely from the vacuum chamber through a thin platinum window out into the air, and I assure you that we have got quite a thrill out of seeing the beam of six-million-volt deuterons making a blue streak through the air for a distance of more than twenty-eight centimeters. Our purpose in bringing the beam out and away from the cyclotron chamber is twofold: partly to make it convenient to carry on scattering experiments, and partly to bring the beam to a target at a considerable distance from the vacuum chamber in order to get rid of the annoying neutron background produced by the circulating ions in the chamber striking various parts of the accelerating system. With this latest improvement in the design of the cyclotron, I think now we have an apparatus which closely approximates one's desires.

"I believe in my last letter I mentioned that we have been carrying on experiments on the biological action of neutron rays. During the past two months such biological matters have taken a good share of my attention, because I feel that such matters, as well as nuclear physics, are of great importance. My brother, Dr. John H. Lawrence of the medical faculty of Yale University, has been out here studying the effects of neutrons on a certain malignant tumor called 'mouse sarcoma 180.' He has compared the lethal effect of neutrons and x rays on the tumor and on healthy mice and has very impressive evidence that this malignant tumor is relatively much more sensitive to neutron radiation than to x-radiation. If this is generally true for malignant tumors, we have here a very important possibility for cancer therapy. I am sure that it will not be long before neutrons will be used in the treatment of human cancer. . . .

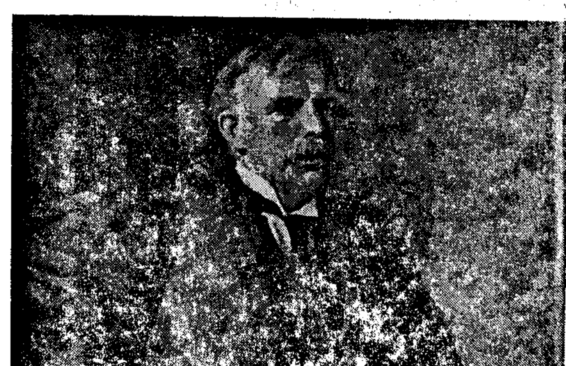
"I was interested to hear that you are beginning the new building for your two-million volt D.C. plant and that you are undertaking the construction of a large magnet.

"I received the letter from Cockcroft and in the next few days will be sending him detailed information.

"Several days ago I received an invitation to attend the meeting in September of the British Association for the Advancement of Science and I have written a tentative acceptance and I can arrange to be away from the laboratory at that time. I should like very much to come to England to spend two weeks. In the event that you should decide to build a cyclotron, it is possible that I could be helpful by going over in detail with you matters of design."

Unfortunately, the design of the cyclotron for the Cavendish Laboratory, and its brother for Chadwick, in Liverpool, did not follow the lines

JOHN LAWRENCE who used cyclotron for medical research, with Ernest, 1927.



ERNEST RUTHERFORD, by Birley.

developed in Berkeley. It was entrusted to a large electrical engineering firm, with no previous experience, while funds were too restricted to enable the magnets to be as large as was desirable. Much trouble was experienced with them, and they never performed as efficiently as the virtual copy of the 60-in. Crocker cyclotron built by us in Birmingham. However, they did useful work, and established the technique in Britain.

NEWS OF HIS NOBEL PRIZE brings joy to Ernest Lawrence, 9 November 1939.

Biology and beam extraction

Lawrence wrote to Rutherford on 24 Nov. 1936:

"I had intended writing you some time ago regarding Dr. R. [Ryokichi] Sagane, who has been with us the past year and desired to spend this year in the Cavendish Laboratory. I am afraid that he has arrived, and therefore words in his behalf now are a bit late. However, I should like to say that we liked Sagane very much; he proved to be a self-reliant and competent experimenter and a congenial personality. I do hope that you will find him an agreeable person to have as a visitor in the Laboratory, for I know that he is very anxious to be with you and will profit a great deal by such a sojourn.

"All of us here are very busy with a number of things. In addition to the nuclear work, we are devoting a lot of attention to biological problems, as I feel that there is important work to be done in this direction as well as in nuclear physics. We are supplying various artificial radioactive substances to the chemists for investigations of chemical problems and to biologists, particularly physiologists, for use as tracers in biological processes. I do hope that in this way we shall be able to contribute to the elucidation of some biological questions. We are also investigating quite extensively the biological effects produced by neutrons. I think we can say pretty definitely now that neutrons do not parallel x rays in their biological action. Studies of the comparative effects of x rays and neutrons will doubtless shed light on the mechanisms whereby ionization produces effects in biological systems, and of course also there are the possibilities of effective medical therapy with neutrons.

"In some preliminary experiments on a mouse sarcoma, we got indications that neutrons had a greater selective action in killing this tumor than x rays. Under separate cover I am sending you a reprint of this work. This fall, similar experiments have been carried out upon a mouse mammary carcinoma with similar indications. In these more recent experiments, many more tumors and mice were irradiated with neutrons and x rays than in the first experiments on the sarcoma, and the new data also indicate a greater selective action of the neutrons on tumor tissue. It seems to me quite probable that neutrons will prove to be valuable in the treatment of cancer.

"We are this year undertaking the establishment of a new laboratory, which might be called a laboratory of medical physics. The organization and planning of the new laboratory is taking a good share of my time this year, but of course I am glad to do it, although I regret I cannot spend full days in the laboratory. Friends of the University have given funds for a new building and equipment, and I hope that by late next fall, experimental work in the new building will get under way. The architects have practically finished the building plans and we are engaged in designing the new cyclotron. Many of us are having pleasure in planning the new apparatus; although doubtless we are deluding ourselves into thinking that the new outfit will be all that a good cyclotron should be.

"For certain experiments in progress we recently further modified our present cyclotron to bring the beam entirely out of the magnetic field, and we are finding the new arrangement one of great convenience for many experiments. I am enclosing a photograph of six microamperes of six million volt deuterons emerging into the air through a platinum window at the end of a tube six feet long. The beam is quite parallel and can be brought out considerably farther if so desired without undue loss of intensity.

"I have heard from several sources that you are very well and very busy—and in view of the latter, I can hardly expect a letter from you, although, needless to say, I should be greatly delighted if you should find time to write a few lines.

"Professor and Mrs. Bohr are coming to Berkeley in March and we all are looking forward to their visit. I wish it were possible to persuade you to visit America also."

Rutherford replied with characteristic enthusiasm for Lawrence's success:

"I got your letter a few days ago, and was very interested to hear of your latest developments in getting a beam of fast particles well outside the chamber. I congratulate you on your success in this difficult task, and I gather you are hopeful to get even stronger beams in this way. The photograph you have sent me is a beautiful one, and I would be very grateful if you would allow me to reproduce it in a lecture I am just publishing called 'Modern Alchemy,' which is an expansion of the Sidgwick Memorial Lecture I gave in Cambridge a few weeks ago. Unless I hear from you to the contrary, I will assume that you agree to this.

"Dr. Sagane visited us this term and he then decided to go for a short tour to Germany and Copenhagen, and is returning here in the New Year to begin some work. He seems a pleasant fellow, but he writes to me that he is finding a difficulty in seeing some of the German laboratories, as it is necessary to get a special permit from the Government to do so. This state of affairs in Nazi-land is rather amusing, and when some of our men from the Cavendish wished to visit Berlin to see Debye's laboratory, he wrote to Cockcroft that official permission would have to be granted by the Government before he could admit them!

"As to our own work, we are going ahead as usual. The new High Tension Laboratory is nearly completed and we hope to get a D.C. potential of 2 million volts going. We are also making arrangements to run one of your cyclotrons in due course.

"We celebrated J. J. Thomson's 80th birthday on December 18th by giving him a dinner and presentation in Trinity and also an address with signatures from many of the Cavendsh people. He is still very alert intellectually, and he was much moved by our little homely address.

"I wish you good luck in the development of your new laboratory and success in your experiments."

Cyclotron radiations

It was on 11 Feb. 1937 that Lawrence wrote again to Rutherford:

"I greatly appreciate your very interesting letter received some time ago. I know that you are extremely busy and it is very kind of you to write at such length.

"Your account of the state of affairs in Germany is almost unbelievable. One would think with such a scientific tradition the German people could not adopt such an absurd course of action in scientific affairs.

"The dinner to J.J. Thomson must have been a very nice occasion. It is certainly fine that he has such vigor at his ripe old age.

"I am glad to hear that your new high tension laboratory is coming along nicely and that you are also constructing a cyclotron. As I have written Cockcroft, if we can be of assistance in any way we should be only too glad. I have just heard that he is coming over for some lectures at Harvard and I have written him a letter inviting him to come out to see us. I do hope it will be possible for him to do so. I think it is possible that he might be saved some unnecessary beginning troubles by spending a few days in our laboratory operating our cyclotron. Also, in a month or so we shall have our new cyclotron chamber for the present magnet practically completed in the shop. This new outfit has quite a few improvements which Cockcroft would probably want to consider in his design.

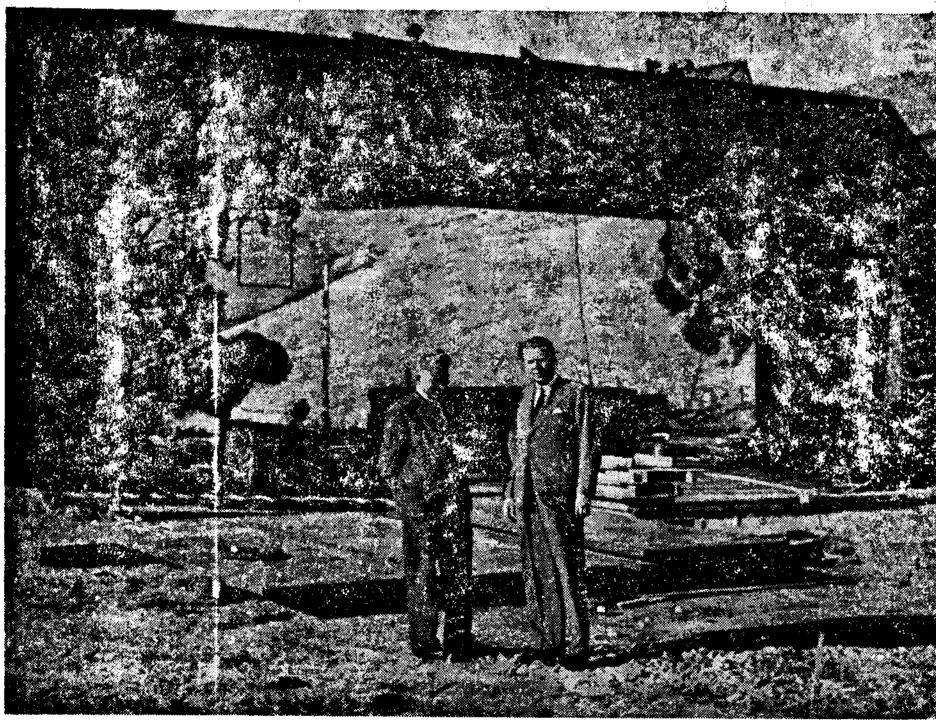
"During the past few weeks we have been bombarding with 11 million volt alpha particles, studying the radioactivities produced. In addition to those already reported we have been findng many new activities, especially on up the periodic table. Also we have been making some absorption measurements of the radiation from the cyclotron and find that there is a very penetrating component. We do not know what it is yet, but the indications are that the penetrating radiation consists simply of very energetic neutrons. A 7 inch thickness of lead does not cut it to half. According to Oppenheimer theoretical considerations indicate that the mean free paths of neutrons vary as their energy. Hence it may be that the 14 MV neutrons from Be + 5 MV D^2 have mean free paths of more than 50 cms—something like the penetration of the radiation observed. We are continuing with the experiments with the endeavor to get the experimental facts as clear-cut and definite as possible, and I am sure when this is done we shall understand what is going on. Under separate cover I am sending you several reprints."

He followed this with a further letter of 24 Feb., having received some reprints of lectures given by Rutherford:

"Thank you very much for the reprints of the lectures, which I have already read with much pleasure and profit. The history that you tell about is certainly absorbing. Your discussion of the essential role played by the development of new methods and techniques in the advance of science appealed to me very much, as I have always held similar views, and of course your mention of the cyclotron in this connection was to me the highest compliment. Your lectures, which I regard as models for us younger men, have a quality in common with your great experimental works, that is to say, they go to the heart of the matter and bring out the essential points with beautiful simplicity. . . .

"We have been pursuing the investigation of the radiations from the cyclotron, and have pretty well satisfied ourselves that there is nothing extraordinary about the radiations excepting that it is an extremely difficult matter to screen out all the neutrons and the gamma rays from any particular region. We have now quite a lot of water around most of the cyclotron, but in spite of that Professor Lewis in the Chemistry building next door is not able to carry on his experiments with his sources of neutrons consisting of a mixture of beryllium with 200 milligrams of radium, and we find that at a distance of 300 feet from the cyclotron the mixture of neutrons and gamma rays from the cyclotron produce an easily detectable ionization. We are now planning to have the cyclotron in the new laboratory in a basement room rather than at ground level in order to cut down the amount of radiation getting out into surrounding laboratories. I am afraid that you will find your new cyclotron something of a nuisance in this regard also."

It is clear that these two enthusiastic men were developing a considerable understanding and respect for one another. Lawrence absorbed more than he realized of the spirit of the father of nuclear physics, and he was able to pass this on to others. The center of gravity of the study of the nucleus was already moving across the Atlantic to the United States, a move which was to become almost complete by the end of the second world war. Rutherford was to write only once more, in reply to the following invitation from Lawrence:



MARK OLIPHANT AND ERNEST LAWRENCE stand before 184-in. cyclotron, 1941.

Invitation to Charter Day

"I have just been talking with the President of the University, who has asked me to write you informally as to whether there would be any possibility that you might be willing to come over here to give a Charter Day address next March or a year later.

"Charter Day here is regarded as a very important occasion and the speaker at the exercise is always someone of great distinction. President [Robert] Sproul is aware that you may be very reluctant to come, but is most anxious to persuade you to do so, since he appreciates your eminence, not only with respect to your scientific contributions but also with respect to your general scientific statesmanship and world-wide good influence. I do hope you will entertain thoughts of coming over, as quite aside from the Charter Day exercises, all of us in the laboratory would gain so much from your visit, even though it were very brief. Needless to say we would do everything we could to make your stay with us pleasant.

"The President is anxious to know whether there is a possibility that you will come, and so if it is not too much trouble, I should appreciate a note from you at your early convenience. In case you should consider coming, it would be helpful if you would give me some informal indication of a suitable financial arrangement which I could transmit to President Sproul, as I know it is customary to provide a proper honorarium. . . .

"We enjoyed very much Cockcroft's visit, brief though it was. I need not describe here what we did when he was with us, as doubtless he has given you a complete report. . . .

"Hoping to hear from you soon and again hoping that you will actually entertain thoughts of coming over next March, and with highest personal esteem, I am

Respectfully yours,"

Rutherford answered:

"I have just received your letter, asking me whether I could visit California next March, in order to be present at your Charter Day Exercises.

"Please convey my thanks to your President for his very kind suggestion and invitation. I write, however, to let you know at once that there is no possibility of a visit next year, as I have already arranged

SKETCH of Ernest Rutherford in 1928.

to go to India in November and preside over a joint meeting of the British Association and the Indian Association of Science, in January, 1938. I shall not return until February, and I shall find great arrears of work to attend to. At this stage, I cannot make any promises about the following year. I have so many calls on my time, that it is difficult for me to make arrangements too far ahead. At the same time, I greatly appreciate the very kind invitation of the University and yourself. I should personally like to have the opportunity of visiting California again, and in particular of seeing something of the work of your laboratory. Cockcroft told me about his visit, and how kind you had been in helping him.

"We are now preparing the foundations for the cyclotron, which we hope will be ready for transmission to Cambridge in July.

"I am glad you were interested in the little book and the lectures I sent you.

With best wishes,

Yours sincerely,"

Lawrence was naturally disappointed that Rutherford could not accept the invitation to Berkeley, but wrote saying that he was glad that the possibility of a visit in the followng year was not ruled out.

Rutherford had looked forward with keen anticipation to the meeting in India. He believed implicitly in the British Commonwealth, and his political liberalism led to his welcoming the development of responsible self-government in India. He had had many Indian students and had known well that remarkable mathematical genius, Srinivasa Ramanujan, also a Fellow of Trinity College, who had died so young, leaving behind a series of intuitive mathematical theorems that intrigued the world of mathematics for the succeeding generation. He spent much time in preparing his presidential address for the occasion. This address contains two passages that are significant in the present context:

"It is imperative that the universities of India should be in a position not only to give sound theoretical and practical instruction in the various branches of science but, what is more difficult, to select from the main body of scientific students those who are to be trained in the methods of research. It is from this relatively small group that we may expect to obtain the future leaders of research both for the universities and for the general research organisations. . . . This is a case where quality is more important than quantity, for experience has shown that the progress of science depends in no small degree on the emergence of men of outstanding capacity for scientific investigation and for stimulating and directing the work of others along fruitful lines. Leaders of this type are rare, but are essential for the success of research organisation. With inefficient leadership, it is as easy to waste money in research as in other branches of human activity. . . ."

Speaking of artificial radioactivity:

"As Fermi and his colleagues have shown, neutrons and particularly slow neutrons are extraordinarily effective in the formation of such radioactive bodies. On account of the absence of charge, the neutron enters freely into the nuclear structure of even the heaviest element and in many cases causes its transmutation. For example, a number of these radio-elements are produced when the heaviest two elements, uranium and thorium, are bombarded by slow neutrons. In the case of uranium, as Hahn and Meitner have shown, the radioactive bodies so formed break up in a succession of stages like the natural radioactive bodies, and give rise to a number of transuranic elements of higher atomic number than uranium (92). These radioactive elements have the chemical properties to be expected from the higher homologues of rhenium, osmium and iridium of atomic numbers 93, 94 and 95."

Rutherford's death

Rutherford was not destined to go to India. He had suffered for years from an umbilical hernia, to relieve which he wore a truss. On 14 Oct. 1937 he became unwell, and was sick enough in the night to be removed from his home to a hospital next afternoon. An operation for Richter's hernia was performed at once, and the outlook appeared good. However, normal bowel movement was never reëstablished, and despite the efforts of his physicians, he died of intestinal paralysis and intoxication on 19 Oct. His great wish at the onset of his illness was to be well in time to fulfil his presidential task in India.

Cockcroft and I were in Italy, at the Galvani Celebrations when news of Rutherford's death reached us. We were very upset and sad. At the morning meeting on 20 Oct., before we left to return to England, Bohr, Rutherford's older student and colleague, who loved Rutherford as we did, spoke movingly of the great man. Afterwards, on 20 Dec., he wrote to Lawrence thanking him for the many kindnesses shown him, Mrs Bohr, and their son, on his recent visit to the Radiation Laboratory, and for his great help in the construction of the cyclotron in Copenhagen. His letter ended:

"When in spite of all this I have not written long before, it has, however, not least been due to the very sudden death of Rutherford which has caused, as you understand, so great upset among his friends. Only a few weeks before I attended his unforgetful dignified funeral in Westminster Abbey, I had visited him in Cambridge where he was as cheerful and enthusiastic over his work as ever. In some way it was the most beautiful end of his marvellous life, but at the same time it makes the feeling of loss ever so acute. Still, I know that the thought of Rutherford will be to you as to myself a lasting source of encouragement and inspiration and will be a close bond between all of us who admired and loved him."

To this Lawrence replied:

"Lord Rutherford's sudden passing . . . was a great shock and your remarks in your letter, which I appreciated so much, are very true. It is sad that Lord Rutherford could not have lived longer, but on the other hand we may rejoice in the memory of his great life. . . .

"These tragic events remind one that life is short and uncertain and that time is not to be wasted. I often think that, (perhaps more so now because of my mother's serious illness) that we know really so little about the biological processes, and we physicists should not pass by any opportunities to be of help in biological research, although perhaps our first inclination would be to devote ourselves to fundamental physical problems."

What happened to the neutron

Rutherford had predicted the existence of the neutron in his Bakerian Lecture to the Royal Society in June 1920. During the following years, sometimes with the aid of research students, he and Chadwick searched diligently for the particle which both were convinced was essential in the structure of the nucleus. Many experiments were made, and James Chadwick has given a charming personal account of these.[2] The elusive neutral particle was discovered by Chadwick in 1932, and its effectiveness as an agent producing nuclear transformations was established soon afterwards by Fermi and others. Rutherford was intrigued by the properties of the neutron, and in his last lecture, read posthumously by James Jeans at the joint congress in India, the passage that I have quoted shows how interested he was in the production by neutrons, in collision with uranium, of the transuranic elements of higher atomic number than any existing naturally on earth. He did not live to experience the excitement created by the discovery by Hahn and Strassmann in 1938, of the fission process, or the beautiful work of Otto Frisch and Lise Meitner, which established clearly that the uranium nucleus could indeed split into two parts when it absorbed a neutron. On 9 Feb. 1939 Lawrence wrote to Cockcroft: "We are having right now a considerable flurry of excitement following Hahn's announcement of the splitting of uranium."

He went on to say that within a day of reading about it in the newspapers, they had observed the heavy ionizing fragments produced in the fission of uranium, and had identified several radioactive species among them by chemical methods.

"We are trying to find out whether neutrons are generally given off in the splitting of uranium, and if so, prospects for useful nuclear energy become very real."

Lawrence was one of the few in the United States who rapidly appreciated the profound significance of the discovery of the fission process. In England the possibility that it had military significance was more quickly realized in particular by Frisch and Rudolf Peierls, and by Chadwick, who showed independently that a fast-neutron fission chain process in the uranium isotope of mass 235, leading to a super-explosion, was possible. In 1941 when I visited Lawrence again, the magnet for his giant cyclotron was being erected on the new site on a hillside above the campus of the university. We discussed the general problem, and in particular the methods that we had been considering in Britain for the separation of the isotopes of uranium. He was deeply impressed by the serious view of scientists in England that nuclear weapons were not only almost certainly possible, but that Germany might be working on the problem. Soon afterwards, he began his experiments upon the separation of the uranium isotope by means of the CALUTRON, a technique which we began to develop independently in my laboratory in Birmingham, using the magnet of the 60-in cyclotron, which was being built with the aid of information generously supplied by Lawrence during and after my visit to Berkeley in 1938. In 1943 this minor effort by us was abandoned in favor of coöperation with Lawrence, and under the arrangements for a joint attack on the problem of nuclear energy, made between the governments of our countries, we moved to Berkeley.

This is not the place to discuss subsequent events, in which Rutherford and his Cavendish Laboratory played no part. If he had lived, he would have rejoiced in the subsequent triumphs of Lawrence and his colleagues in the Radiation Laboratory. But he would have regretted that his nuclear atom had become of such practical importance that the main motives for the financial support of such work, in all countries, became other than the advance of knowledge of nature.

It was a great privilege to be the pupil and colleague of Rutherford, and to have known, and worked with that other Ernest who so ably took over the torch of nuclear physics from him, and carried it to further heights of achievement. Rutherford, the greater scientist, laid the foundations of modern physics. Lawrence, with his greater flair for technology and organization, showed how to build, on those foundations, the massive edifice of physics today. All who knew and worked with these great men shared deep respect for their genius. But they inspired more than that. The warmth of their natures, their generosity, and their simple, unassuming personalities, generated an abiding love that made our lives fuller and happier.

Acknowledgements

The author is grateful for the ready access given him to correspondence and papers in the Cambridge University Library and in the Lawrence Radiation Laboratory. He acknowledges the help given personally by Sir James Chadwick, Sir John Cockcroft, Mrs Molly Lawrence, John Lawrence, Robert Brode, Leonard Loeb, Raymond Birge, Edwin McMillan, Robert Thornton, Harold Fidler, Mrs Eleanor Davisson, Daniel Wilkes, and many others. Luis Alvarez suggested that the article be submitted for publication to PHYSICS TODAY.

The sketch of Rutherford by R. Schwabe is from a copy presented to the author by Lord Rutherford.

The portion of Birge's history of the Berkeley physics department covering the period 1868 to 1932 will be available in mimeograph form (in limited number) within the next few months.

(This is the second of two articles on Ernest Rutherford and Ernest Lawrence. The first appeared in the last issue.)

Reference

2. J. Chadwick, Ithaca 26 VIII, 2 IX (1962).